Reply to Comments of Reviewer 1 on manuscript acp-2013-437:

General Comments:

To my knowledge, most all other published analyses of the time series of stratospheric ozone measurements have been focused on determinations of changes in their trends with the goal of showing that a slow ozone recovery has been underway since about 1997. No one else seems to have been concerned about whether or not the associated profile ozone responses to the solar uv-forcing are truly consistent across the time series of the several long-term ozone datasets or more importantly whether or not there are other decadal-scale forcings that may be affecting those analyzed solar cycle responses and trends. This latter, more limited objective was a goal of my original study (Remsberg and Lingenfelser, RL). Because the SAGE II dataset has been recently reprocessed to version 7 (v7), I felt obliged to re-analyze the SAGE II v7 ozone in the same way as in RL and report my findings to the research community in the ACP journal where RL appeared. That is the primary goal of my current manuscript, and I will attempt to state that objective more clearly in my revision. I conclude that the SAGE II v7 ozone data are of better quality than those of v6.2. In addition, I note that the reviewers of RL did not find that my original statistical analysis approach was unconvincing.

I can agree that I did not really show that the SC-like response profiles from SAGE II and HALOE are truly equivalent. This incompleteness of my study is because both the MERRA temperature time series (archived as part of the SAGE II v7 dataset) and the NOAA CPC temperatures below about 40 km (that are part of the HALOE dataset) exhibit discontinuities that can affect the trends and SC-like responses of the ozone, when one converts number density versus altitude (SAGE II ozone units) to mixing ratio versus pressure (HALOE ozone units) or the other way around. To my mind, a better indication of the expected relationship between ozone number density and mixing ratio was presented in the model study of Dhomse et al. (2011) based on the temperatures in the ERA re-analyses, although admittedly dependent on the quality of the ERA values. In my revised manuscript I will show an example plot of the discontinuity in the SAGE II v7 (MERRA-based) temperature time series near the stratopause that is very likely affecting the SAGE II ozone response profile at that altitude, or where disagreements in the ozone response profiles from HALOE and SAGE II are still apparent. I will also point more clearly to the findings of Dhomse et al. (2011), regarding the relationship from the respective ozone quantities of the SAGE II and HALOE datasets.

Specific Comments:

1. Kyrola et al. (ACPD, published 23 April 2013 and in review for ACP) conducted an analysis of the SAGE II v7 ozone plus the GOMOS ozone in the manner that you are suggesting for my study. As with most recent ozone studies, they have focused on the ozone trends. I essentially agree with their findings and do not see any value in simply repeating their analyses. Contrary to your assertion that my analysis approach using periodic and linear trend terms (and no proxy terms) has negative consequences, I would argue that I have been able to uncover important anomalies in the ozone time series that have been blurred by the “more conventional” analysis

approach. In particular, my decision to analyze two separate, but overlapping 14-year ozone time series has actually exposed the decadal-scale, dynamical effects more clearly—which was my primary goal. Furthermore, my analyses of the SAGE II ozone time series from 1984-98 was intentional in that I wanted to see whether I could overcome the effects of the end point ozone anomalies that ensued from the Pinatubo event. An outcome of that analysis was the finding of an unrelated ozone anomaly in 1989-90 in the tropical middle atmosphere that appears to be a reinforcement of the QBO signal due to a perturbation from an extended ENSO event.

2. As I indicated in my reply in General Comments above, I will provide evidence in my revised manuscript of a discontinuity in 1999 in the temperature time series of the lower mesosphere that can impart a bias from the algorithm for the removal of Rayleigh scatter effects in the ozone measurements from SAGE II and subsequently for its ozone responses at those altitudes. There are no apparent discontinuities in the temperature time series of 1984-98 that might alter its ozone response profiles in the same way. The temperature discontinuity in 1999 should be corrected in the upcoming version of MERRA II, and hopefully in a subsequent reprocessing of the SAGE II ozone.

As in the study of RL, I also conducted analyses of time series from late 1992-2005, in order to look for end point effects from Pinatubo in my analyses of the 1991-2005 time series. Contrary to your assertion, I did not conduct parametric studies to settle on a specific 1992-2005 period "to yield the best possible agreement". I merely followed the lead of Lee and Smith (2003) for avoiding the possible effects from Pinatubo.

As you note, I have not commented on similar findings about the ozone response profiles from the SBUV ozone. I feel that it is premature for me to consider the findings from SBUV. The current SBUV version 8.6 data supersedes the ozone version used for the analyses reported in WMO (2007). I judge that it is better for others to analyze the merged v8.6 ozone time series and to report on its SC-like responses and trends. I will check the literature to see what they have reported in this regard and will note their findings in my revised manuscript. However, I realize that their results are unlikely to be specific to the two 14-yr time spans that I have chosen for my study and that I may be unable to make direct comparisons with my findings based on their work.

3. I will revise my abstract summary to emphasize that I find that the SAGE II v7 data is of better quality than that of v6.2 for evaluating the interannual and decadal-scale variations in ozone and its trends, which is the primary purpose of my study. This finding is a result of the much smaller residuals that remain after accounting for the seasonal terms in the v7 ozone. I will make note of the fact that there are some remaining discrepancies in the ozone responses from SAGE II and HALOE, especially at the higher altitudes. I will also relate their responses to any findings in the literature for the SBUV v8.6 ozone. Finally though, Thompson et al. (2012) have recently reported that there are still some nagging uncertainties in the stratospheric temperature time series, even those used in the analyses of Frame and Gray (2010).

Technical Corrections:

4. As in my reply to specific item 1, Kyrola et al. (2013) already analyzed the SAGE II v7 data in this way and have reported their findings.

5. You are correct that Frame and Gray (2010) only report on their analyses of solar cycle signals in temperature and winds. I'll remove this reference at this point on page 3.

6. I will add a brief justification for not extending the SAGE II analyses to lower altitudes. First, there is the issue of the large amount of aerosol extinction in its visible ozone channel following Pinatubo and the much larger estimates of ozone error, as a result. Secondly, the period of the QBO forcing is more variable in the lower stratosphere, such that adopting a QBO term with a fixed period for my MLR model is no longer expected. On the other hand, in the middle and upper stratosphere the QBO signal is very regular and has a period very near to 28 months. This term in my model fits the data of those altitudes very well.

7. Generally, I favor showing confidence intervals for the presence of a term in my MLR models (Figures 2-7 and 9-10), instead of generating standard deviation error bars for profiles after taking their average across several latitude bins (for Figures 12-13). The use of error bars in this instance tends to imply a level of certainly that is often unwarranted, I think. Such error estimates for the SC-like response and the trends only apply if there is truly no remaining structure in the residuals from the fit of a given MLR model and/or no end point anomaly issues.